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ROYAL GARDENS, KEW.

BULLETIN

OF

MISCELLANEOUS INFORMATION.

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[1891.

CLXXXII.—WEST AFRICAN BASS FIBRE.

Raphia vinifera, P. de Beauv.

This palm is described in the *Flore d'Oware et de Benin*, vol. I. p. 76 (tab. 44, fig. I. et tabb. 45 and 46), from which the following notes have been extracted. It is very abundant on the borders of rivers intersecting the countries near the sea in the kingdoms of Oware and Benin. The tree is of medium height, having leaves of from 6 to 7 feet or more in length with spiny leaflets. The fruiting spadix is very large, about 4 feet long, and forms a heavy load for one man to carry. What this tree lacks in height is compensated for by the beauty of its form, the brightness of its colour, and its imbricated shining fruits. The stems are used to form the framework of native dwellings, and the leaves, bound with lianes, are used for thatching. Huts so built are substantial, and afford a good protection from the rain and heat of the sun, but at the same time serve as a haunt for vipers, rats, and other vermin. From the trunk an intoxicating beverage of a whitish colour is obtained, and is called by the natives "Bourdon"; it is not quite so sweet as ordinary palm wine, but is more vinous, and appears to contain a larger quantity of spirit. The fruits of this palm, which are collected all the year round, are likewise

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1891.

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said to afford a beverage of a second quality which will keep for a considerable time.

The *Board of Trade Journal* [November 1890 p. 596] quotes from the *Lagos Weekly Times* an account of the fibre, and states that it "promises to become an important and valuable addition to the exports from Lagos."

It will probably be found a useful substitute for Piassava, an account of which was given in the October Bulletin for 1889.

The following correspondence has taken place between this establishment and the Colonial Office on the subject.

COLONIAL OFFICE to ROYAL GARDENS, KEW.

SIR,

Downing Street, October 3, 1890.

I AM directed by Lord Knutsford to transmit to you a copy of a despatch from the Governor of Lagos, reporting that he had forwarded to you, for analysis and report, a box containing 20 lbs. of bass fibre obtained from the *Raphia vinifera*.

Lord Knutsford will be glad to be informed of the result of your examination of the parcel in question.

I am, &c.

(Signed) EDWARD WINGFIELD.

The Director, Royal Gardens, Kew.

SIR A. MOLONEY to COLONIAL OFFICE.

Government House, Lagos,

August 20, 1890.

MY LORD,

I HAVE the honour to report that there will go forward by the next mail steamer addressed to the Royal Gardens, Kew, a box containing 20 lbs. of a bass fibre obtained from the *Raphia vinifera* or wine palm of West Tropical Africa.

2. The sample I had hurriedly prepared during my last visit to the eastern district.

3. On its "find," area of supply, local preparation and uses, as also on its prospects as an export for which I anticipate a bright and profitable future, I have drawn up a minute with a view to its advertisement.

4. May I ask your Lordship to let the Director of the Royal Gardens, Kew, who takes such deep interest and gives such encouragement to the development of the economic botany of our colonies, have a copy of this despatch, and to allow of the circulation of the minute among the various Chambers of Commerce.

5. I attach a copy of the notice that has locally appeared inviting the attention of the public to this fibre.

I have, &c.

(Signed) ALFRED MOLONEY,
Governor.

The Right Hon.
Lord Knutsford, G.C.M.G., &c.

EXTRACT of MINUTE by the Governor of Lagos on the Bass fibre of the Bamboo palm (*Raphia vinifera*).

In a letter received by me, shortly after my arrival in Lagos in February last, from a well-known Manchester firm, a sample of a fibre known as "African Bass" was forwarded, with the following remarks:—

"If this can be found and shipped in quantity I could sell large quantities. It should be kept straight, tied up first in small bundles, thickness of a man's wrist, and these made up into bales of about half a hundredweight each. It must be kept straight, whatever the length, as the bends spoil the fibre and makes its difficult to work."

* * * *

"Please note the brown fully mature fibre is preferable to the light red colour; present value 30*l.* to 32*l.* per ton."

In the "African Bass" of which the sample was sent to me I was surprised to recognise one of the commonest of the native fibres of this colony, used, I may say, by every fisherman in the manufacture of his lines, and prepared from one of the most plentiful of the palm trees of the colony, the *Raphia vinifera*, or "Bamboo" palm.

The "African Bass" is in appearance a stiff and wiry fibre, varying in colour from dark brown to light red, dependent for its shades on duration of soaking. It is most readily obtained in lengths of from 3 feet to 4 feet, beyond which length it is inconvenient to pack and difficult to procure without injury to the tree. In diameter it varies from $\frac{1}{16}$ to $\frac{1}{30}$ of an inch, the latter of which may be accepted as the limit of fineness to be admitted in a commercial sample for the European market. It is used, I believe, mainly in the manufacture of hard brushes for various domestic and manufacturing purposes. The demand appears to be very large, and the price, as shown above, is exceedingly satisfactory.

The source of its supply in this colony alone may be said to be practically inexhaustible, as will readily be acknowledged when its origin is explained.

The "Bamboo" palm, or *Raphia vinifera*, is perhaps the commonest tree in the swamps and low lands which line the waterways of the colony. Dense thickets of these palms, traversed only by the palm wine gatherer or the bamboo cutter, push their way into the lagoons, and extend over the flood grounds, and even to a distance of from 15 to 20 miles up the river valleys into the interior. The area occupied by these *Raphia* forests it would be impossible to calculate, but it may be accepted without doubt that they extend throughout the length of the colony, and to a distance of at least 15 miles from the sea coast, and that over this area of about 5,000 square miles they form a considerable proportion of the vegetation, next only in numbers to the Oil palm (*Elæis guineensis*) and the Mangrove (*Rhizophora mucronata*). The fact that one can steam for miles, as I have frequently done, wondering to what commercial advantage they could be put, through uninterrupted *Raphia* groves on either hand in the Eastern waters, impresses one with the extent of the acreage which must be overrun by this graceful palm.

Everybody in the colony is aware of the manifold uses of the *Raphia* palm; how from its leaves hats, cloth, and cordage are made, from its leaf-stems rafters, fences, and walls, and from its crown of young unopened leaves palm wine of excellent quality. Of one part only the use seems not generally known, and it would appear that this particular

portion of the tree, though hitherto treated as useless, is in reality of more value than all the rest.

When the "Bamboo" cutter clears away the leaves from the lower stem of the palms the trees present a very ragged and uneven appearance, owing to the practice of leaving a portion of the leaf-stalk adhering to the parent stem. These base-stalks partially encase the bole of the tree and project upward and outwards, forming the scaly covering which gives so strange an appearance to a grove of *Raphia* palms. From these stumps of the leaf-stalks the native fishing lines are made. The fibre is extracted by a process of soaking and scraping, which is exceedingly simple and is fully understood by every bamboo cutter and line maker. It is this fibre which is known in the European market as "African Bass," and there is no apparent reason why with a population who are in the habit of preparing it, and a source of supply which may be regarded as practically unlimited, we should not be able to compete on even terms with the sources of supply which at present monopolise the market.

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In the Yoruba language the *Raphia vinifera* is known as *Igi-oguro*, *Eriko* and *Akpako*, the Bass fibre as *Iyo*, and the fishing line *Iyo-oguro* or *Iyo-agbe*. Along the inland waters or lagoons from Popo to Mahin the natives use this fibre for fishing lines, and as twine and rope.

MESSRS. IDE AND CHRISTIE to ROYAL GARDENS, KEW.

72, Mark Lane, London, E.C.,

October 10, 1890.

DEAR SIR,

WE duly received your letter of 8th instant, and to-day brings us the sample of fibrous material found at the base of the leaves of the "Bamboo" palm of West Africa (*Raphia vinifera*).

We have seen this material before, and the small importations that were made (some years ago) were not at all well received by consumers, who found that the expense of cleaning and the ultimate result were most unsatisfactory when compared with the price and results of South American Piassava.

During the past year or two, however, the market values of the latter have undergone a great change, Bahia Piassava having largely risen in price. We think, therefore, that it might be well worthy of producers' attention were they to select only these fibres of which we return you specimens herewith, and clean them effectively from all the adherent small fibre. Only the strong healthy fibres should be selected and prepared, and the various lengths should be bundled separately.

The material being somewhat of a novelty, it might be well for shippers to confine their first shipment to, say, 10 to 20 tons, so as to try the market adequately. If properly selected and cleaned we estimate that it would sell at 25*l.* per ton to-day in London, a figure that, in our opinion, should leave a handsome profit to the producer.

Yours, &c.

(Signed) IDE AND CHRISTIE.

D. Morris, Esq., M.A., F.L.S.

MESSRS. IDE AND CHRISTIE to ROYAL GARDENS, KEW.

72, Mark Lane, E.C.,

October 24, 1890.

DEAR SIR,

WE have your favour of the 23rd instant, and duly note the contents of enclosed extract from Minute of Governor of Lagos on the fibre of *Raphia vinifera*.

Since we last wrote to you on this subject, a few bales of "African Bast" recently imported have been sold and reached the extreme price of 42*l*. The fibre had been carefully selected and remarkably well cleaned, hence the excellent market it met with. We scarcely expect this price would be maintained for substantial quantities, but for fibre of equal merit the immediate outlook would seem to indicate that 35*l*. to 40*l*. might be the range of value.

Yours, &c.

(Signed) IDE AND CHRISTIE.

D. Morris, Esq., M.A., F.L.S.

CLXXXIII.—CHINESE GINGER.

Alpinia Galanga, Willd.

Every one is familiar with the preserved ginger which comes to our dessert tables from China, and is imported in earthenware jars of more or less artistic merit. As long ago as 1878, Dr. Perceval Wright, Professor of Botany in the University of Dublin, wrote to Kew, pointing out that the "large flat finger-like masses sent to this country from China as preserved ginger," differed from anything that the ordinary ginger plant (*Zingiber officinale*) could possibly produce. The fact itself was obvious as soon as it was pointed out.

The question then naturally arose what was the plant used by the Chinese. Ginger itself, as is the case with so many cultivated plants, is unknown in a wild state. But there can be little doubt that it is a native of Asia. It was known as a spice to the Greeks and Romans, who received it by way of the Red Sea and supposed it to be a production of southern Arabia. It was very early introduced into the West Indies, from which it was shipped for commercial purposes to Europe as early as the 16th century. The dried ginger met with in British commerce is almost entirely derived from the West Indies, Sierra Leone, Egypt, and India. It is noteworthy that we receive none from China.

At the instance of Professor Perceval Wright, Mr. G. H. M. Playfair (of H. M. Chinese Consular service) sent a Warden case of the Chinese plant to Kew in 1878. The plants were propagated without difficulty and largely distributed to tropical colonies. Unfortunately this brought us no nearer the solution of the difficulty. That the plant was different to ordinary ginger was obvious. But plants of the ginger family, as is the case with many plants which are easily propagated by their rhizomes, are shy of flowering in cultivation, while their foliage is all so much alike that it affords no adequate means of discrimination. The plant obstinately refused to flower at Kew, as it also appears to have done in Ceylon, and there was nothing to do but wait till some lucky chance as to conditions of growth in some colonial garden enabled Kew to be furnished with the necessary material for identification.

In the meantime, a root of a ginger from Siam shown in the Health Exhibition at South Kensington in 1884 proved to be alive when received at the Kew Museum. As it obviously differed from the ginger of the shops it was handed over to the assistant curator, Mr. Watson, for growing. He succeeded in raising plants from it, and ultimately in flowering it in 1886, when he communicated the following account to the *Gardeners' Chronicle* (July 31, 1886, p. 150).

“*Siam Ginger*.—Amongst the collection of fruits, &c. shown by the Siam Commission at the International Health Exhibition, held at South Kensington in 1884, were some roots labelled ‘Ginger.’ These were obtained for the Kew Museum, but one of them being alive was planted to grow, and it is now bearing stems 5 feet high, and is in flower. On comparing it with the drawings and specimens in the Herbarium, Mr. Baker has identified it with a specimen labelled ‘*Alpinia* sp., Bangkok,’ which was collected by Sir R. Schomburgh in 1864, and which is very near to *A. Allughas*, also a native of Siam, where, according to Schomburgh, it is cultivated for its Cardamomum-like fruits, and is known as *Luk-Reu* or Bastard Cardamom. Under the name of Galangal, *A. officinarum*, a Chinese species is cultivated for the sake of its aromatic rhizomes, and this unnamed species now in flower at Kew is apparently largely cultivated by the Siamese as a substitute for Ginger. The rhizome is very thick, slightly flattened, and not so freely branched as in common Ginger; it has the pungent aromatic properties of Ginger, so far at least as could be told by tasting it. There is some reason for believing that the Chinese Ginger of commerce is not obtained from *Zingiber officinale*, the source of Jamaica Ginger, but from a species of *Alpinia*, and possibly this unnamed one at Kew. Plants of true Chinese Ginger are now growing at Kew, and these may soon flower; at present they have the same habit, broad leaf and rhizome, of the Siam plant. In *Zingiber officinale* the inflorescence is borne on a separate short stem without leaves, the barren stems being about 3 feet high, and clothed with narrow spear-shaped foliage; in *Alpinia* the flowers are borne in panicles on the ends of the stout leaf-stems, the well known *A. nutans* and the newer *A. mutica*, both beautiful garden plants, being familiar examples.”

In 1887 Sir Joseph Hooker figured the plant in the *Botanical Magazine* (tab. 6944) as a new species, *Alpinia zingiberina*. The rhizome he describes as “very aromatic;” they “smell and taste a good deal like the official plant; they are, however, very much larger, at least three times as thick, are much more shortly and irregularly branched, and the branches are thickened in the middle.”

Mr. Baker in working up the *Scitamineæ* for the *Flora of British India* has arrived at the conclusion that *Alpinia zingiberina* is not essentially distinct from *Alpinia Galanga*, Willd., the Greater Galangal, a plant originally native of Java and Sumatra now much cultivated in India for its rhizome.

In December 1888 we received the first authentic flowering specimen of the “Chinese Ginger” from the Botanical Department, Jamaica. It was grown at the Hope Gardens, by the Superintendent, Mr. Harris. This proved, as suspected by Mr. Watson, to be an *Alpinia*, and also identical with the Siam plant. It is in fact nothing more than the well known *Alpinia Galanga*.

Specimens received in the present year from the indefatigable correspondent of Kew in Dominica, Dr. Alford Nicholls, F.L.S., confirm this result, and Mr. Ford, the Superintendent of the Botanical and Affores-

tation Department, Hong Kong, having flowered the plant in the Hong Kong Botanic Garden, arrives at the same conclusion.

Professor PERCEVAL WRIGHT to ROYAL GARDENS, KEW.

Trinity College, Dublin,
June 4, 1878.

MY DEAR DYER,

FOR some time I have been anxious to know the species of plant which produces the large flat finger-like masses sent to this country from China as preserved ginger. I never found the Jamaica ginger produce such massive stems. I accordingly wrote early in this year to an old pupil, Mr. Playfair, who is in the Consulate at Swatow, to collect me all the information he could, and this morning I got a letter from Swatow, April 25th, 1878, saying that by the same mail that brings it, a friend of his, Mr. T. W. Richardson, sails, "bringing in a Wardian case half a dozen of the young ginger shoots, which he will on arrival in London take or send to Kew. I have given him a letter to Sir J. D. Hooker asking the latter to forward the plants to you, and suggesting that if all survive, he should, if he cared for them, keep one or two at Kew."

Should they turn up and prove all sickly, will you kindly ask that they be all cared for at Kew before coming on another journey.

Yours, &c.

(Signed) E. P. WRIGHT.

G. M. H. PLAYFAIR, Esq., to ROYAL GARDENS, KEW.

H.B.M. Consulate, Swatow,
April 22, 1878.

DEAR SIR,

I HAVE been requested by Dr. E. P. Wright, Professor of Botany, Trinity College, Dublin, to send him some specimens of the plant from the root of which the preserve, known as "Chinese Ginger," is made. By good fortune a gentleman at this port, Mr. T. W. Richardson, is on the eve of his departure for England, taking with him various plants in a Wardian case, and he has most kindly offered to take six young ginger plants among them to be transmitted to you, as Dr. Wright suggested, for transmission to Dublin.

Should the whole or the greater portion of these plants survive the journey, I hope you will not scruple to detain some of them for the Kew collection if they are required, and I shall feel much obliged if you will be so kind as to forward the remainder to Dr. Wright, at Dublin.

Later I hope to find a convenient opportunity of sending tubers, leaves, flowers, &c. of the mature plant preserved in salt and water, or otherwise, for the Dublin Botanical Museum, and I shall again venture on the liberty of addressing the packages to your care.

I forward this letter favoured by Mr. Richardson.

Yours, &c.

(Signed) G. M. H. PLAYFAIR.

Sir J. D. Hooker, K.C.S.I., C.B.

G. M. H. PLAYFAIR, ESQ., to ROYAL GARDENS, KEW.

H.B.M. Consulate, Amoy,

April 10, 1885.

MY DEAR MR. DYER,

I REMEMBER perfectly sending home those roots of *Zingiber* in a Wardian case, in 1878, to Dr. Edward Perceval Wright. If my memory serves me, I did not send any plants or parts of plants in salt and water. What Dr. Wright wanted was such parts of the plant as could be utilized for the purposes of botanical identification, such as the flowers, or, failing these, at any rate the fruit. But it has been established as incontrovertible by Dr. Hance that the ginger plant in China never flowers. He has been in the country since 1861, and is not likely to be mistaken; the natives, moreover, assert the same. The plant is propagated entirely by tubers, I believe. It is hopeless, therefore, to hope to identify it fully. Dr. Hance told me in 1878, when I made inquiries on the subject at the instance of Dr. E. P. Wright, that, in his opinion, the Chinese plant is *Zingiber officinale*, and identical with West Indian ginger. I may state that he has seen a flowering *Zingiber*, but it was a wild plant, and not the variety used for the manufacture of sweetmeats. An application to him would, doubtless, elicit more detailed information on the point.

Canton is the head-quarters of preserving ginger; the plant grows here too, and is used as a condiment, but not as a sweetmeat. As I said above, it is, to my knowledge, impossible to obtain either a flowering or fruiting specimen. If you would let me know whether the leaves and tubers alone would be of any use to you, I should be happy to procure any quantity; but, apprehending that such would be no practical utility, I shall await further orders.

Yours, &c.,

(Signed)

G. M. H. PLAYFAIR.

W. T. Thiselton Dyer, Esq., C.M.G.

EXTRACT from Dr. Trimen's Report on the Royal Botanic Gardens, Pérádeniya, for 1885, p. 12.

Chinese Ginger.—Of this plant, the source of the *preserved* ginger of commerce, nothing is certainly known. In China, where it is grown, it is said never to flower. I have received from Kew a box of roots, which are growing well at Pérádeniya. The plant is clearly quite distinct from ordinary ginger, and I await its flowering with interest.

EXTRACT from Mr. Ford's Report on the Hong Kong Botanical and Afforestation Department for 1886, Appendix I.

Some doubt has existed as to whether the Chinese have not one or more kinds of plants in use as ginger that are unknown elsewhere. I have taken steps for collecting together and cultivating all the kinds of plants generally included by the Chinese as ginger with the hope that when in cultivation they can be studied and observed in such a manner as to secure all possible information in connexion with this subject.

While at San Ui I was fortunate in being able to obtain from cultivated plants good flowering specimens. These I dried, and together

with specimens of the roots (properly rhizomes) forwarded to the Director of Kew Gardens for a study of them to be made there, where they can be compared with other kinds, or with specimens of the same kind from other places.

The specimens which I procured were, without doubt, *Zingiber officinale*, the species commonly in cultivation in other parts of the world.

It is, however, possible that some other plant, which is not a true ginger, may be used in making the celebrated Canton preserved ginger, but all the information which I have yet obtained points to the species *Zingiber officinale* as the only kind which the Chinese use for this purpose.

The ginger cultivated on the Lo-Fau Mountains has a wide reputation amongst the Chinese as being of unusual efficacy in medicine; this superior quality may, however, be derived merely from peculiarity of soil or climate, which communicate to the plant exceptional properties.

DR. H. A. ALFORD NICHOLLS, F.L.S., to ROYAL GARDENS, KEW.

Extract.

Dominica, West Indies,

July 5, 1890.

MY DEAR MR. MORRIS,

I HAVE succeeded in flowering the Chinese Ginger at Saint Aroment, and I send you a botanical specimen (taken from a shoot fully $4\frac{1}{2}$ feet high) by this mail. You will be enabled from this specimen to determine the plant, and I hope I may be first in the field. I was really the first to get the Yoruba indigo to flower, but foolishly I did not send on specimens to Kew. I have other specimens of Chinese Ginger flowers pressing, and you can have them if you wish.

Yours, &c.

(Signed) H. A. ALFORD NICHOLLS.

SUPERINTENDENT, BOTANICAL AND AFFORESTATION DEPARTMENT,
HONG KONG, to ROYAL GARDENS, KEW.

(Extract.)

DEAR SIR,

Hong Kong, July 10, 1890.

THE "Chinese Ginger" which you had at Kew, and sent to Ceylon, from which Dr. Trimen, at my request, sent me a piece about two years ago, has just flowered with me, and it is setting some fruit. It turns out to be *Alpinia Galanga*. I shall have more particulars to write about this when the fruit has matured, when I will write fully on the subject. Dr. Trimen informed me that it would not fruit in Ceylon.

Yours, &c.

W. T. Thiselton Dyer, Esq., C.M.G.

(Signed) CHARLES FORD.

CLXXXIV.—PRODUCTION OF SEED AND SEMINAL VARIATION IN THE SUGAR-CANE.

Mr. Darwin sums up the results of many well-known observations when he remarks (*The Variation of Animals and Plants under Domestication*, Vol. II., p. 168), "Plants which from any cause grow too luxuriously, and produce leaves, stems, runners, suckers, tubers, bulbs, &c. in excess, sometimes do not flower, or if they flower do not yield seed." As an example he gives (p. 169) the sugar-cane. This, "which grows vigorously and produces a large supply of succulent stems, never, according to various observers, bears seed in the West Indies, Malaga, India, Cochin China, or the Malay Archipelago."

No one has ever found the sugar-cane growing wild, and no one, says Alphonse De Candolle, in his well-known "*Origine des Plantes Cultivées*" (p. 125), has ever described or figured the seed. The late Mr. Bentham always spoke to me of the seed of the sugar-cane as a thing entirely unknown in herbaria. Hackel, the most recent authority on grasses, states in his recently published *Monograph of the Andropogoneæ* with respect to the seed of the sugar-cane, "*cariopsin nemo adhuc videsse videtur.*"

The experience of practical men has been to the same effect. Leonard Wray ("*Practical Sugar Planter*," 1848, pp. 29-33), discusses the question at great length. A few passages may be quoted.

"We often hear of 'cane seed,' and latterly a very earnest inquiry was set on foot with a view to decide the question whether the sugar-cane is really raised from seed in any part of the world or not, which terminated, I believe, in establishing the fact of there being no country known wherein the cane is, at present, raised from seed; whatever may have been the case in earlier ages."

* * * * *

"The constantly recurring idea that canes are raised from seed in Egypt and the East Indies has kept alive a strong belief that the plant could be much improved by skilful cultivation and care, if this said seed could be obtained by European agriculturists. Hence numerous have been the endeavours, both private and public, which have been made to become possessed of it. The Royal Agricultural Society of Jamaica took up the subject, and exhibited much industry in collecting information; and, for aught I know to the contrary, may still be pursuing the inquiry."

* * * * *

He finally arrives at the conclusion, "no variety of sugar-cane is known to perfect its seed (or, indeed, to produce anything like seed), either in India, China, the Straits of Malacca, Egypt, or even in the South Sea Islands; as in all those countries the cane is entirely propagated by cuttings." Lock, Wigner, and Harland (*Sugar Growing and Refining*, 1885), authorities nearly 40 years later, may be quoted to the same effect (p. 61):—

"It has more than once been stated that the sugar-cane is in some localities reproduced from seed, but the statement has originated in a misconception, there being no kind of sugar-cane known to regularly perfect its seed. Propagation is, therefore, effected exclusively by means of cuttings from the stems."

The number of varieties of sugar-cane in existence is considerable; in the Jamaica Botanic Gardens in 1884 there were 60 under experimental cultivation. The interesting question arises as to how they have

originated. Looking at the fact that the raising of sugar-canes from seed has long been apparently unknown, the conclusion seems most probable that these varieties have slowly developed by the selection of what are known as bud variations. That is, a cultivator has noticed a cane in a clump which seemed to exhibit some advantageous difference from its fellows, and then by a repetition of the process, a distinct variety has ultimately been reached.

In 1886 a correspondence took place between Kew and the Colonial Office as to the measures which might be taken to improve the sugar-cane, as undoubtedly the sugar beet had been improved, by persistent analysis and selection. As to the desirability of the attempt, and the importance of the results which would flow from it if successful, there could be no matter of doubt. But an institution such as Kew can only advise according to the materials that exist for its scientific judgment. In the case of the sugar beet, the cultivator dealt with a plant which could run through its life in a comparatively short space of time, and could again and again be reproduced from seed. Taking advantage of the well-known principle of "seminal variation" year by year, an appreciable advance could be made towards the desired end. But in the case of the sugar-cane, there was nothing to indicate at the time, that anything of the kind could be done. All the evidence pointed to the fact that the sugar-cane had lost the power of producing seed. Under the circumstances, there was nothing but what is called "bud variation" to work upon. The advice given to the Colonial Office in this sense seems to have been transmitted to the several sugar-growing colonies, and to have been embodied in official notices. The following appeared in "The Barbadoes Agricultural Gazette" for August 1886:—

ROYAL GARDENS, KEW, to COLONIAL OFFICE.

[*Extract.*]

Royal Gardens, Kew, May 13, 1886.

"Mr. Thiselton Dyer deems it advisable to direct the attention, not only of professional botanists, but also of planters, to the fact that new varieties in sugar-canes are to be sought in bud variation appearing accidentally in the cane fields, and that when such bud varieties are noticed, stock plants should be raised and carefully experimented upon until their value is fully known."

These announcements led to several communications being received at Kew with reference to the supposed seeding of the sugar-cane. Thus, in 1887, a correspondent in Fiji sent what he evidently believed to be the true thing, but which turned out to be Guinea cork (*Sorghum vulgare*).

The most important communication was, however, one from Mr. J. B. Harrison, the Island Professor of Chemistry and Agricultural Science at Barbados. This was dated September 17, 1888, and was published in the "Kew Bulletin" for December of the same year. For reasons which will appear in the sequel it is now reprinted.

Prof. HARRISON to ROYAL GARDENS, KEW.

Government Laboratory, Barbados,
17th September, 1888.

On certain of the higher districts of the island from time to time growths of sugar cane resembling fine grass have been noticed, but in most cases no attempts have been made to cultivate them. Mr. Parris some years ago succeeded in raising a few canes from the cane arrow or

flowering shoot. Mr. Clarke did the same with the arrow of the purple transparent cane, but did not succeed in getting the seedlings to flourish, and my wife's father many years ago succeeded in getting the arrows to produce young canes, but not in cultivating them. Knowing these cases, Mr. Bovell and myself considered that a favourable opportunity of examining into this question offered itself during the cultivation of the varieties of canes which we have here. These canes were planted in rows of four broad by 25 feet deep, and so as to have two sets of each kind, in all 36 plots of 18 varieties, planted side by side. The plots were noticeable this year for the number of arrows sent up by some of the varieties. We gave strict orders to the labourers employed in weeding and watching the adjacent land to report to us any grasses springing up upon them in any way differing from the usual weeds. Towards the end of January they reported to us that a few tufts of grass different to the usual kinds were making their appearance. We found these to be growing in a rather narrow belt of the field on one side of the plots and in a little below it, following the direction of the prevailing wind. They were found not only on the surface of the field, but also on the bottom of a drain which had been dug in the field to a depth of 18 inches. Some 80 or 90 plants sprang up at intervals afterwards. We found a good deal of difficulty in keeping them alive, as the sun quickly shrivelled them up; it was necessary to protect them in many cases from the direct rays of the sun and to keep them constantly watered. In this way we succeeded in saving some 64 or 65 plants. Of these we carefully examined three or four so as to ascertain as far we could the absence of any particle of old cane in them. Their mode of growth was quite different to that of canes growing from the eyes of canes. Sixty plants were successfully transplanted, and are being cultivated. At present they are not far enough advanced in their growth to speak with certainty, but there appears to be amongst them several different kinds, probably five or six at the least. If you think it worth while, Mr. Bovell and myself will send you a specimen cane of each sort in January or February next, when they will be sufficiently far advanced to show their characteristics. The way in which they first grow is quite sufficient to account for them not being often noticed upon the fields. The weather here during January last was particularly favourable for their growth, and the fact of different varieties being grown side by side is, of course, much more favourable for the production of seed than the growth of one variety only. I have never heard of the Bourbon cane producing here fertile arrows; in all the alleged cases of fertility the arrows were either those of the purple or white transparent varieties which, as you are aware, are prone to variation. We shall again attempt this year to obtain the same results. I am anxious to have the benefit of your opinion upon this year's results, as of course, if we can establish the fact of the cane occasionally, and, under certain favourable conditions, producing fertile seed, it will open an important field of investigation.

J. B. HARRISON.

The interesting discovery of Messrs. Harrison and Bovell gave distinction to the important work in the scientific investigation of the conditions of culture of the sugar-cane, which as far as I know alone in the Empire these gentlemen had for some years carried on at Dodd's Reformatory, Barbados. Attention had already been drawn to the "zeal and capability" with which this work had been prosecuted, in the "Kew Bulletin" for June 1887 (p. 9).

The importance of the discovery that it was possible to raise seedling sugar-canes, attracted a good deal of attention in the West Indies. Its practical significance was clearly summed up in an article in the "Demerara Argosy" for April 13, 1889. I extract the following passage:—

"From what we know of the great improvement that has taken place in other cultivated plants, through selection in seminal generation, the discovery can hardly prove other than important and beneficial. An improved variety on the best we now have may not be obtained, possibly for several years, but we may be confident that if the better kinds do, or can be induced to breed, with systematic attempts, carefully conducted to control the operation, and careful trial and selection of the progeny improved forms will in time result."

The same view was expressed in a letter from Kew to the Colonial Office, August 9, 1898, ("Kew Bulletin," 1889, pp. 242, 244).

"From the point of view of the sugar planter it is a fact which, if established and intelligibly followed up, is capable of effecting as much improvement in the sugar-cane and in its yield of sugar, as has been effected in the beet."

Scientific men are never content to accept a fact on mere statement. Before the seminal reproduction could be regarded as more than highly probable at Kew, it was necessary to see authentic specimens of the seeds from Barbados, and ascertain that plants of the sugar-cane could be raised from them. It is well known that grasses occasionally produce barren inflorescences, which bear vegetative bulbils instead of seeds, and as Messrs. Harrison and Bovell do not profess to be botanists, it would have been no discredit to them if they had been misled by such a circumstance. In fact, Dr. Fressanges of Mauritius, seems to have fallen into this error, to judge from the account given in the "Journal of Botany" for October, 1890, pp. 303-305.

This doubt was, however, dissipated by the receipt from them at Kew of portions of the arrow (flowering panicle) of the sugar-canes supposed to yield the seedlings. They were carefully studied by the Assistant Director, Mr. Morris, and I take the following account of the result from a letter communicated by him to the "European Mail" for August 20, of the present year.

"Some hundreds of spikelets were carefully examined, but it was only after a lengthened examination that a few seeds were found . . . Some of the seeds were sown under suitable conditions, and the whole process of germination carefully watched. Ultimately these observations were repeated over and over again during a period extending over several weeks "and at last the fact was fully proved by a series of microscopical "preparations, which definitely settled the matter, beyond doubt or "question."

As the matter had an important botanical besides a commercial side, Mr. Morris communicated the facts of Messrs. Harrison and Bovell's discovery and his own observations in confirmation to the Linnean Society on March 10. This paper with a plate was not published till December 15. A matter of so much interest was not likely to pass unnoticed. Some report of the paper, in no way authorised, circulated in the newspapers and ultimately reached the eyes of Mr. Harrison, who in the meantime, on the recommendation of Kew, had been promoted to another post in Demerara.

This led to the following communications being received from him.

GOVERNMENT CHEMIST, British Guiana, to ASSISTANT DIRECTOR,
Royal Gardens, Kew.

Government Laboratory, Demerara,

August 28, 1890.

SIR,

MY attention has been called on many occasions lately by my friends in England to the many paragraphs which have appeared not only in the daily papers but in scientific ones, in which the whole credit of an investigation which you did not originate or in any way take part in is ascribed to you. As I believed that you would make an opportunity of correcting such mis-statements, up to the present I have not taken steps to publicly right Mr. Bovell and myself.

These statements originated in the Manchester press, have thence been copied and spread through the majority of English papers, and appear to have arisen from the paper read before the Linnean Society by yourself, in which judging only by the published accounts, while you gave us credit for growing self-sown seedlings you apparently omitted to mention the further stages of our investigation as detailed in our report for 1889 a copy of which you have received.

In the "Manchester Examiner and Times" of July 29th, appears a long article headed "Sugar Cane Seed (from a Correspondent)." In this article appear statements which are absolutely false, and which are so manifestly unfair to those who originated and carried out the investigation in question that I am forced to take notice of it.

I have therefore addressed a letter to the Editor of that paper which if he declines to publish I shall take steps to have published and circulated. I enclose a copy of it for your information.

I may add that much indignation is felt in the West Indies by what is there considered your having tacitly allowed the credit due to the Java investigators and to Dodd's Experimental Station to be entirely ascribed to yourself.

Trusting that before you receive this letter you will have taken steps to correct these false impressions and so render the publication of my letter unnecessary.

I am, &c.

D. Morris, Esq., F.L.S.,
Royal Gardens, Kew.

(Signed) J. B. HARRISON.

P.S.—I have written the above and the enclosed letter entirely on my own responsibility, and have not consulted Mr. Bovell on the subject.

[Enclosure.]

To the Editor of the "Manchester Examiner and Times."

Government Laboratory, British Guiana,

August 22, 1890.

SIR,

MY attention has been drawn by friends in England to an article headed "Sugar Cane Seed (from a Correspondent)" which appeared in your paper under date of July 29th. As many of the statements in that article are absolutely untrue, I must ask you to kindly allow me space to correct them.

The statement that Mr. Morris, of Kew, has long held the opinion that there are seeds by which the sugar-cane could be grown, is strikingly at variance with his published ones. On the 12th May 1886, Mr. Morris wrote to the Colonial Office pointing out that as the sugar-cane

did not produce seed, attention should be directed to bud variations which might appear accidentally in the fields. Next in a paper on the West Indies, read by Mr. Morris in March 1888, before the London Chamber of Commerce, that gentleman again stated that canes could not produce seed, and repeated his advice about bud variations. At that time we had at Dodds, seedling canes three months old, and the late Dr. Soltwedel, of Java, had proved conclusively in 1887 that the cane did produce fertile seed, and had published his results in the "Tydschrift voor Land."

Later in the year I received from Mr. Morris a copy of his paper, and after reading it, wrote to him on September 17th, 1888, on behalf of Mr. Bovell and myself, asking him if it was not somewhat indiscreet to commit himself to such a statement in the face of the fact that in Barbados many planters were satisfied that canes could be grown from seeds, that several instances existed in which canes had been grown from seeds, and also shortly describing the results obtained at Dodds.

This letter which we regarded, and still regard, as a private one, did not receive from Mr. Morris the courtesy of either acknowledgment or reply, but to our astonishment and indignation certain portions of it were published by him in the "Kew Bulletin" for December 1888. Our first intimation of this was by seeing the footnote headed "Seedlings of Sugar Cane" on page 11 of the "Times" Weekly Edition for December 1888.

In the notes added to our letter in the Kew Bulletin, Mr. Morris most carefully reserved his final opinion.

In our letter of September 17th, we asked Mr. Morris for the benefit of his opinion and advice, but have received neither advice nor assistance from him.

The next statement, that "under his instructions I buried the flowering head of a cane in the ground," is absolutely false. No such instructions were ever given by Mr. Morris, and most certainly if I had buried a cane arrow in the ground no plants would ever have grown from it.

In January 1889, spikelets of the cane were stripped from the arrows by Mr. Bovell and sown. Of these many contained fertile seeds, which germinated and grew, and certain of the germinated seeds were preserved as microscopic objects.

On August 9th, 1889, Mr. Morris wrote to the Colonial Office asking to be supplied with seedling canes for experimental cultivation at Kew, and also pointing out the importance of obtaining specimens of what is known to be mature seed of the sugar-cane, and of placing such specimens for examination in the Kew Herbarium. He also under date of 16th October wrote to me stating that they "were very anxious to obtain specimens of sugar-cane arrows *with seed* for the National Herbarium," and asked for assistance in this matter "as soon as possible." In compliance with these requests, by the first mail in December 1889, we sent to Mr. Morris a bottle of cane spikelets containing, as we had already proved by germination experiments, fertile seeds, and at the same time to assist him in his examination of the spikelets we sent him specimens of seeds and of seedlings in various stages of germination which we had preserved in glycerine.

From the seeds which he thus obtained, Mr. Morris has raised seedlings, and has also described and figured the actual seeds. At the same time apparently he has tacitly allowed the credit belonging to an investigation which he neither originated nor in any assisted in to be entirely ascribed to himself.

But the question still remains, may not Mr. Morris have been the first to figure and describe the fruit of the sugar-cane? The answer to this question is emphatically, no.

In 1889, some months before Mr. Morris read his paper at the Linnean Society, Dr. Benecke of the Samarang Sugar Experiment Station had published a monograph entitled "*Over Suikerreit uit Zaad*" in which he not only published in some detail Dr. Soltwedel's researches, but fully described the fruit of the sugar-cane, and gave a series of illustrations of the seeds of the cane in their different stages of germination, and of the seedlings in the various stages of their development. I can vouch for the absolute accuracy of these drawings. Probably Mr. Morris had not received a copy of this work, which is written in Dutch, when he described the fruits of the sugar-cane for the first time in English, but if he had had one, it would have greatly assisted him in his work.

As a matter of fact, no new discovery has been made. Canes were successfully grown from seedlings in 1858 by the Honourable J. W. Parris of Barbados, and in 1860-61, were raised from the seed itself by Mr. F. P. Carter of that island. On account of the very low fertility of the seeds, and also of certain objectionable characteristics that arise in the canes (and which still characterise many of the seedlings raised at Dodds and elsewhere) the matter was allowed to drop by the planters. Whether or not any advantage will be gained by the use of the seed, is still a vexed question, as some authorities expect that the canes raised from seed may not contain as high a per-centage of sugar as those grown from the cuttings.

What has been done in this matter in recent years, is that to the late Dr. Soltwedel is due the credit of being the first (in recent years) to grow canes from seeds, but he appears to have ceased from his investigations as useless when he found that the seedlings were equally subject to disease as the canes grown in the usual way; to the Dodd's Experiment Station in Barbados, that of having worked out the method of successfully raising canes from seeds in large numbers, and of having shown the great tendency to variation which exists amongst canes so raised, and of having distributed large numbers of seedling canes and seeds through the West Indies and to Kew; to Dr. Benecke of having first described and figured the fruit of the sugar-cane; and to Mr. Morris that of having first grown canes from seeds in England, and of first describing and figuring the fruits in English. His work has doubtless done much to spread the knowledge of the fertility of the cane seed, but to allude to him as the discoverer of such fertility is not only grossly inaccurate but unfair to those scientists who have independently of him worked out that fact.

By his letters and writings previously to December 1888 [he] did far more to prevent, by discouraging research in that direction, the proof of the cane's seminal fertility than to assist in it.

I am, &c.

(Signed) J. B. HARRISON,
Government Chemist, British Guiana,
formerly Island Professor of Chemistry
and Agricultural Science, Barbados,
and Natural Science Scholar of
Christ's College, Cambridge.

It is evident that Mr. Harrison thinks that he has been badly used by Kew. Nothing, it need be said, was farther from our intention or desire. And no doubt, had Mr. Harrison confined himself to private

remonstrance it would have been easy to have satisfied him on that point. But as his view of the case has been communicated to the public prints, and practically amounts to a charge of bad faith against a member of the Kew staff, it seems desirable to record Mr. Harrison's claims along with the other facts of an interesting history.

A very few points in his letter to the "Manchester Examiner" seem to require a word of comment.

i. Mr. Harrison complains of the views expressed by Mr. Morris in official communications to the Colonial Office on the general subject of the improvement in the sugar-cane. It is only fair to say that these letters were written in my name and under my instructions, and that I am officially responsible for the views they express.

ii. Mr. Harrison complains of the publication of his letter of September 17, 1888, announcing the discovery of the seminal fertility of the sugar-cane in the Kew Bulletin for December, as he regards it as a private communication. I did not do so; I regarded it as an official communication from a Government official on a matter of very great importance, which had for some time engaged our attention, and I directed its publication.

iii. The accusation that Mr. Morris or any member of the Kew staff "tacitly allowed the credit belonging to an investigation which he neither originated nor in any way assisted in, to be entirely ascribed to himself" seems to me preposterous, in the face of the publication of Mr. Harrison's statements of his own results in these pages. The newspaper press of the United Kingdom is large, and no one can be held responsible for what is stated in it by unauthorised persons. Nothing whatever was known at Kew of the article in the "Manchester Examiner" nor down to this day do we possess a copy of it.

iv. What Mr. Morris, and for that matter Kew, has done in investigating the matter, is quite correctly stated by Mr. Harrison.

A copy of Dr. Benecke's pamphlet did not reach Kew till August 13, 1890. The history of Soltwedel's work is, however, given in such a convenient form in a letter from Mr. H. Winter, the former director of the Samarang Experimental Station, to Mr. George Stade of Berlin, published in the extremely useful journal, the "Sugar Cane," for December last, that I do not hesitate to reproduce it. Mr. Winter was "on the spot during most of the time when the experiments were being made by Dr. Soltwedel."

H. WINTER, Esq., to G. STADE, Esq.

DEAR SIR,

Berlin.

YOU have asked me, with reference to the statements in the *Sugar Cane* for September, respecting the priority of the discovery of seed from the sugar-cane, to say a few words on the question. I am glad to respond to the request, as I was an eye-witness during several years of the attempts which were made at the Samarang Station in Java to grow canes from seed, and the first discoverer of the seed is now no longer able to speak for himself.*

Mr. Morris's supposition, that "the publication of the Java experiments will probably now claim precedence over all others with which

* Dr. Soltwedel died December 17th, 1889, from syncope of the heart.

"we are at present acquainted," is quite correct. To the experimental station at Samarang belongs, not only the honour of having been the first to obtain seed from the cane and cane from seed, but also of having furnished the first detailed scientific description of the seed and the process of germination.

As regards the establishment of the first point, the fact does not appear to be as yet generally known that Dr. F. Soltwedel, at that time director of the experimental station in question, was already in possession of seed of the true sugar-cane a year before Messrs. Harrison and Bovell. At any rate, Mr. G. Dureau, in an article "*Les Graines de Canne à Sucre*" (*Journal des Fabricants de Sucre*, for 16th April 1890), makes no mention of this, and the editor of the *Sugar Cane* expresses himself doubtfully on the point. The following are the words of Dr. Benecke in his treatise "*Over suikerriet uit zaad*," being a summary of Dr. Soltwedel's original communication :—

As early as the spring of 1885, Dr. Soltwedel had commenced, at the Bendokerep factory at Japara, his investigations with regard to the fructification, principally of *Saccharum spontaneum*, L. (Glagah). He had observed that normal pollen grains were present in this species, which developed further on the pistil, and that fertilisation also took place resulting in the formation of actual seed. He had already succeeded, in 1885, in inducing these seeds* to germinate and obtained from them proper plants. In the same year Soltwedel was also engaged in studying the flowers of two varieties of *Saccharum officinarum*, L., viz., Tebbe Cheribon (Java), and Teboe Poetih (Bendokerep). In this case he also met with perfectly developed flowers, but observed neither germination of the pollen grains nor fructification. Although the reports of other investigators offered little encouragement to seek for seed from the first-named kind, yet Dr. Soltwedel was impelled to do so by seeing bees flying so often to the tassels of this cane. He assumed from this fact that the latter must contain something that enticed the bees, and further reasoned that the plants must be furnished with this means of attraction for the benefit of their fructification. Hence he did not allow himself to be deterred by a series of unsuccessful experiments.

Dr. Soltwedel continued his observations in the experiment field at Samarang from April to July 1886. In this year also he failed to find any seed formation in any variety of the true sugar-cane, but in the case of *Saccharum Glonggong* (a kind of sugar-cane growing wild in Java, in the same manner as Glagah, and also not cultivated), seeds of which he induced to germinate, he obtained individuals which were exactly similar to the mother plant.

In the year 1887 Soltwedel finally attained his object. As he formerly observed in the case of Loethers, a variety very rich in sugar, fertilisation without formation of seed, he first of all tried to obtain seed by crossing it with a wild kind which produced seed. Here we cannot refrain from reproducing the original report of Soltwedel, which appeared in the *Tijdschrift voor Land en Tuinbouw*, etc., of the 1st July 1887, in order to show how minutely he went into the question. He writes : "For the purposes of this experiment of cross-fertilisation " we this year selected Glagah and Loethers ; Glonggong was out of the " question, because this kind of cane does not come into flower until " Loethers has done flowering. But the endeavour to produce cross- " fertilisation is accompanied, in the case of the sugar-cane, with almost " endless difficulties, because of the extreme minuteness of the flowers.

* Dr. B. uses, instead of seeds, the more accurate botanical form "fruits."

"By the aid of very small anatomical scissors we endeavoured to cut away the, as yet, unopened staminal buds from a number of flowers of Glagah and Loethers. After so doing we put into the thus partially sterilised flowers of Glagah, pollen dust of Loethers and *vice versâ*."

As a matter of fact it was not possible to ascertain, later on, whether this cross-fertilisation had succeeded or not, however, almost at the same time, fertilisation was observed in the case of 20 varieties of the true cane other than the Loethers variety. Indeed in this year the formation of seed was also detected in nine different varieties. Soltwedel has given details of these in the following table :

VARIETIES OF SUGAR-CANE PRODUCING SEED.

Name.	Whence obtained.	Per-centage of Flowers which Formed Seed.	Weight of a Seed Grain in Milligrams.	Per-centage of Germinating Seeds.
Yellow cane - -	Hawaii -	3.0	0.20	16
Teboe batoeng - -	Borneo -	6.0	0.16	15
Teboe koening - -	Borneo -	4.5	0.10	6
Branche blanche - -	Mauritius -	31.0	0.15	35
Loethers - -	Mauritius -	0.37	0.20	—
Teboe rapooh - -	Java -	0.23	0.22	—
Teboe soerat balie - -	Java -	0.36	0.20	—
Teboe soerat redjoe - -	Java -	13.7	0.11	3
Teboe idjoe - -	Java -	0.8	0.20	20
Glonggong - -	Java (wild) -	8.5	0.16	—
Glagah - -	Java (wild) -	24.0	0.34	—

"Thus, in a spikelet of Branche blanche we found the greatest number of seeds, out of 100 flowers there were 31 on the average which had formed one seed each. In the case of Teboe rapooh we found the fewest seeds, as there was only one seed to 435 flowers. The seeds are exceedingly small, those of Glagah being the largest, and those of Teboe koening and Teboe soerat redjoe the smallest."

A very large number of the seedlings perished ; only from the yellow Hawaii cane did Soltwedel (in 1887) obtain *strong plants which grew to 2½ mètres*. In the year 1888 he divided these into cuttings, and obtained from them in the following year plants of 3½ metres high.

From what has been stated, there can be no doubt as to the man to whom belongs the honour of the discovery of the sugar-cane seed. Without wishing to hurt the feelings of the West Indian investigators or to diminish their universally acknowledged merits, we cannot help pointing out the difference between Soltwedel's successful striving for an object as to which he was certain, and (if we are to judge by the published reports) the more accidental discovery of Harrison and Bovell, and declaring that at least equal acknowledgment with that which they receive is due to the German investigator. As far back as 1837, Soltwedel was actually in possession of the seeds, whilst the Barbados investigators had in 1888 only arrived at a conclusion (which was still open to dispute) as to the existence of seeds from germinating cane plants, and indeed had probably only examined the pistil in the cane flower for seed, without being acquainted with the seed itself.

Further, besides Soltwedel, another planter in Java, Dr. L. Ostermann, had in June 1887 obtained seedling plants by sowing whole tassels in moist soil (Benecke, page 51). Afterwards a chemist named Schmitz, Dr. Ostermann, and more especially Dr. F. Benecke, obtained numerous

plants by sowing isolated seed grains, the whole of which the writer had the opportunity of seeing.

I have already mentioned at the outset that the first scientific description of the sugar-cane seed had issued from the Samarang Station, in the shape of the pamphlet of Dr. F. Benecke, who was labouring there along with Soltwedel as botanist. This work, which Morris characterises as "a very clear and exhaustive account of the whole matter, illustrated by excellent drawings and dissections," was begun in May 1889, and finished somewhere about September of the same year. The translation (the MS. was written in German), execution of the drawings, and printing were surrounded with very special difficulties, so that the latter was not completed until January 1890.

So much towards clearing up the question as to priority of discovery. I may add, in conclusion, that in Java itself people do not attribute to the discovery of the seed that importance for the practical culture of the cane which they appear to do in the West Indies and other countries.

I hope I have met your wishes in this matter, and remain

Yours fraternally,

H. WINTER,

As will be seen, opinions differ as to "whether or not any advantage will be gained by the use of the seed." An excellent account of what has so far been accomplished in British Guiana is given in the "Demerara Argosy" for November 1, 1890. This is worth reprinting, if only for the valuable observations it records on the seminal variation of the sugar-cane.

SEEDLING SUGAR-CANES.

The systematic propagation of the sugar-cane from seed is as yet in its infancy, nevertheless some facts of interest and importance have been discovered which are worth briefly summarising here. Since the time that the re-discovery, at Dodd's Botanical Station, Barbados, of the seminal fertility of the cane was authenticated, realising its potential importance, systematic experimental work has been carried on at our own Botanic Gardens, as our columns have before disclosed. That the very earliest varieties of sugar-cane can reach maturity the first year of their growth from seed has been shown as possible under favourable circumstances this season at the Botanic Gardens; though it must be admitted that even with these very earliest varieties both the proportion of canes in a stool and of plants to a bed of the same variety which flower the first year is small. Seed of the variety *Karakarawa*, which is one of the two earliest kinds in the Colony, was sown on 1st October last year. Three months later the young plants were taken from the seed boxes and pricked out in baskets, five or six in each basket. Six weeks later again they were shifted on singly into larger baskets, which were about six inches deep and wide, and in which they remained till they were from 1 to 1½ feet high, when, on the 19th April last, they were planted out in the open ground. At that time each plant consisted of a solitary shoot, none having begun to sprout from the base. A few weeks later, however, they began to tiller freely and to grow rapidly, and by the middle of September a few shoots of the more advanced plants were in flower, thus completing the cycle of growth. As, when they were planted out in the ground in April, the young plants were only in an equivalent stage to that of a cane top put into the ground at the same time, the record above given shows that seedling sugar-canes of the earlier varieties make rapid and vigorous growth once they get

past the tedious period of infancy, which occupies from four to six months. Only, however, the very earliest varieties mature the first year; all the rest, though they may be only a month or so later in their period of flowering, miss the first season of arrowing, and consequently have to go on to the following autumn before the chance of performing that function occurs again; so that for the great majority of varieties it may be said that two years are required from the time the seed was sown for the seedlings to mature, or from 15 to 18 months from the time they were strong enough to be planted out in the open ground. This is a sufficient proof, if any were needed, that the idea of resorting to seed for propagation in field agriculture is impracticable, and that the present methods of propagation by cuttings or stumps will have to be adhered to in the future as in the past, if only for economy in time. But, of course, the insuperable obstacle to using seed in field propagation, even if time could be regarded, as the Indian regards it, as of no consequence whatever, is the delicacy and slow growth of the sugar-cane in infancy. In the climate of Guiana, field propagation of the cane by seed would probably not yield an average of one plant per acre. As we have emphasised before in these columns, the only useful way of employing the knowledge we now possess of the sugar-cane's seminal fertility, is in nursery propagation, with the object of obtaining new varieties. Of this method of propagation and its results we have now had two seasons' experience, and short as the period is over which the experiments have extended, the information gained is highly interesting and instructive. Numerous facts, naturally of varying importance and practical interest, have been discovered, two of which are of great value because of the promise they hold out of ultimate economic improvement in the sugar-cane by this method of propagation. The first of these important revelations is the wide variation that occurs in seminal generation in the sugar-cane; and the second the marked tendency to improvement shown in this variation. As was naturally to be expected there are numerous instances of retrogression, but the general tendency is clearly on the lines of improvement in each particular variety that has been so far successfully tested. The progress in improvement is gradual of course, and the degree naturally in direct proportion and relation to the character and quality of the parent canes. The inferior varieties, for instance, do not produce large varieties in a single generation, but as with better kinds, few or many of the progeny show a decided improvement on the parent stock. In the majority of instances the improvement, though evident, is not great, but occasionally an instance occurs that is a striking advance. This is the ground of encouragement in pursuing this method of reproduction. If among the seedlings of a variety is found in the first generation a plant twice or thrice the size of the parent plant, we are justified in expecting that by selecting this larger plant and breeding from it again we shall get still further improvement, to be repeated again and again in succeeding generations. This, we may note, is one of the points already achieved. By recording the name of the variety from which each lot of seed was gathered, when it was sown last year at the Botanic Gardens, this possibility of improvement has been established with certainty, so far as the evidence afforded by a few varieties in a single generation can be taken as a guide, and we know of no reason against its acceptance. We have mentioned the occasional improvement observed in size in the seedlings of these carefully recorded parent varieties; but the variation in colour and form and other external characters is more general and conspicuous than that in size. In the majority of instances there is an evident approximation in physical and morphological features to the

characteristics of the parent, but the departure from this typical state covers the entire range of variation possible, from the least to the widest extreme. Of this wide range too many instances have occurred to leave any room for doubt on the ground of possible error of record as to parentage that might arise from mistakes made in gathering and sowing the seed, or in the subsequent course of growth. In many instances the variation is slight, but in several cases purely white or purely green canes have been produced by dark purple ones. We have said that in the majority of the seedlings there is a general approximation to parental likeness, but absolute likeness does not bear a very great proportion to the whole variation. The degree of approximation varies, however, in the different kinds, the smaller inferior canes producing a larger number like the parents than the larger and superior ones. Of one of the larger kinds of which many plants were raised, not one appears to have come absolutely true. This variety, called *Mani*, is a long-jointed claret-coloured cane, and the widest departure it has made in its varied-coloured progeny is to a short-jointed white cane. If this instance stood alone, as we have before intimated, it might be thought that a stray seed of some other kind had got into the stock, but, as we have also before intimated, the several instances of the kind that have occurred are too many to require or to justify resort to any such explanation. It seems not improbable in fact that if any one of the inferior varieties, possessing a fair degree of germinating power upon which success largely depends, were taken in hand, that by seminal propagation and careful selection from the seedlings, as many varieties, showing as wide a range of size, colour, quality, &c., might be procured from it in a few generations, as all the kinds of canes collected from all sugar-growing countries, which we now possess, show! This impression, gathered from our brief experience, justifies sanguine hopes of improvement in the future of the varieties we now cultivate, and imposes the obligation of zealous endeavour to realise this improvement. Another encouraging feature of seedling canes is that the features they first present appear not to be their best or permanent ones. During the first two years, at least, the period seedlings have been under observation here, there seems to be a steady general improvement in character. Where the first shoots have been slender, subsequent ones have come double the size, and where in the earlier canes the joints have been short and often bearded, in the later ones they have been long and clean, and a higher gloss and glow have also developed in the colour. So that as two full years are in most instances required for seedling canes to reach maturity, so also this period is necessary for them to develop their true character. The inference may be taken from this that if the cultivation of the seedling canes first discovered in Barbados had been extended, the objectionable features for which they were discarded and destroyed would have disappeared; in which case we should probably not now have to lament the loss in the experimental working of this potent discovery of the 30 years which have elapsed since that time. Though we have described the wide variation and improvement that might possibly be realised in a few years in breeding and selecting from one of the poorer varieties of cane, we have intimated as well that our object—that is obtaining an improved variety of cane to any now existing—would no doubt be earliest realised by breeding from the better kinds. The primary qualities required are—first, large size, to give weight of cane from the fields, and—second, sweetness to give yield of sugar from the canes. These may be regarded as the primary and absolutely essential qualities of a good cane, while it is admitted at the same time there are

several important minor qualities, which, though dispensable, are also desirable. These pertain to the habit of growth, resistance to drought, earliness, flowering or non-flowering, degree of itch to the leaves, of fibre in the cane, &c., &c. In selecting breeding stock it must be admitted that we have much to learn on the subject of the transmission of qualities in seminal generation in the cane. We have described to some extent the evidence we at present possess of the considerable variation that takes place in descent in the physical and morphological features of the cane; whether the sweetness will vary to the same extent sufficient time has not elapsed yet for us to determine. This character is equally as important as that of the size and physical nature of the cane, and in our experimental work demands equal attention and regard. The few analyses it has been possible yet to make seem to point to the fact that external physical variation is accompanied by internal chemical variation. But we must wait another year, till our pedigreed seedlings have matured, before we can speak with assurance on this point. However, the uncertainty need not affect our present action; there can be no question, whether the saccharine quality varies or not in descent, the best stock to breed from is that possessing in largest degree the qualities, separately or combined, of weight of cane and of sugar. There are three possible ways in which we may look for improvement in the sugar-cane. We may obtain a variety that will give a larger weight of cane per acre, though it may contain no more sugar per ton of cane than the better kinds we possess now. Again, we may obtain a variety yielding a higher per-centage of sugar, though the weight of cane be not increased. This is what has been accomplished in regard to the sugar beet. Thirdly, we may obtain a cane containing both increased yield of cane and of sugar. Which would be the most desirable would depend upon the degree and relative proportions of bulk and per-centage of sugar in the cane. Sufficient has been said to show what is to be sought in propagating the sugar-cane from seed. We shall not get an ideal cane; but Mr. Neville Lubbock gave the writer one day a rough ideal toward which we may successfully strive. "What we want," said he, pointing to a plantain stalk 15 ft. high and as thick as one's thigh, "is a cane as big as that containing 20 per cent. of sugar." Whether we shall ever attain to the size of cane or degree of sugar contents just mentioned may be left to conjecture and the result of future work, but it is along the lines leading to those ends that we confidently look to make progress. The *Scard* seedling gives us an indication of what is possible. This is a plant of unknown parentage and fortuitous birth in Barbados. It was among the earliest natural seedlings discovered there by Messrs. Harrison and Bovell. When an inch or so high, and when nothing whatever could be known of its character beyond the facts mentioned, it was taken by chance from the nursery at Dodds by Mr. Bovell and sent here. It has turned out an entirely unknown variety, and a giant of its race. A few months ago it fell down of its own weight, when ten canes were broken off which collectively weighed 122 lbs. without the tops. In spite of this loss it is still 18 feet high and 12 feet in girth of stool, possessing 24 canes which will probably weigh when cut 200 lbs., half as many more having been killed by borers,—pests that have been unusually prevalent this year.

To sum up. Though the work done in Java undoubtedly anticipated that done in the West Indies by Messrs. Harrison and Bovell, it attracted but little general attention. The discovery of these latter gentlemen has been termed "accidental." Even if true, that is no demerit. Most

discoveries in some sort are accidental. They often lie, so to speak, under our eyes, and only reveal their significance to those who are ready to appreciate it. This Messrs. Harrison and Bovell did, and the greatest credit is due to them for the fact. All that Kew has done in the matter was to put it on record and give it a scientific verification. For my part, I have no doubt, looking at the whole history of the improvement of cultivated plants, that the discovery, for so I think it, of Messrs. Harrison and Bovell has been the starting point of a new era in the cultivation of the sugar-cane, and with time and patience I do not see why even the aspirations of Mr. Neville Lubbock should not be realised. But it will require both.

At the moment of sending this paper to press I have received the following important communication from the Director of Forests and Botanical Gardens, Mauritius. It shows that the advice given from Kew in 1886, as to the possibility of effecting something for the improvement of the sugar-cane by taking advantage of "bud variation," though based upon theoretical considerations, was fully justified. I do not, however, pit one method against the other. I simply point out that the cultivator in quest of new varieties has, so to speak, two strings to his bow.

W. T. THISELTON DYER.

ROYAL BOTANICAL GARDENS, MAURITIUS, to ROYAL GARDENS, KEW.

The Gardens, Curepipe, Mauritius,

9th December 1890.

DEAR SIR,

* * * * * Raising canes from seed to get improved varieties will be a long and tedious affair, and there will be many disappointments before a really good hardy sugar-yielding variety will be obtained. I think it probable that more and better results will be obtained by good cultivation and by new varieties from bud sports. Of these last we have eight or nine in Mauritius alone, some of them are very fine canes and they are extensively planted. Most of them are hardier than their parents and yield more sugar. They are mostly obtained from new canes recently introduced. The sudden change of climate, soil, and other circumstances cause them to be thrown off. More of them might be obtained if the planters were more observing than they are, and closely followed the cane cutters when cutting the canes. Thus they would range all their fields over, perhaps, areas amounting to 1,500 acres, matching each cane as it is seen cut. As things are, a new variety is only observed should it chance to spring up in an outside row.

Yours, &c.

(Signed) JOHN HORNE.

W. T. Thiselton Dyer, Esq., C.M.G., F.R.S.
